tc-2016-243 – Reply to Reviewers

Please find below a detailed, point-to-point answer to both RC1 and RC2, in which the reviewers' comments appear in red, while our replies are written in black. We are attaching a diff file that shows in detail the changes we made in the manuscript.

Reply to RC1

The manuscript "Regional modeling of the Shirase Drainage Basin, East Antarctica: Full-Stokes vs. shallow-ice dynamics" by H. Seddik et al. presents simulations of the Shirase Drainage Basin …

We wish to thank the reviewer for his/her efforts, even though we largely disagree with the assessment. For details please see below.

I am concerned about the novelty of this paper. As stated by the authors in the paper, the Shirase glacier is "one of the fastest flowing glaciers in Antarctica" and its flow is "dominated by sliding". However, the authors also explain that the shallow-ice approximation "assumes that grounded ice flow is governed only by ice pressure and the vertical shear". Using the shallow-ice approximation for modeling such a glacier is not valid here is therefore absolutely no reason to compare full-Stokes and shallow-ice simulations for this glacier.

Note that we have not only modelled the Shirase Glacier, but the entire drainage basin all the way up to Dome Fuji (see our Fig. 1). This is a huge area of ~ 200,000 km^2, large enough to qualify as a stand-alone ice sheet if it were not connected to an even bigger ice body. In most of it, slow, vertical-shear-dominated ice flow prevails (now mentioned in the beginning of the introduction). The regions where it is a priori clear that the shallow-ice approximation (SIA) is problematic constitute only a very small part of the drainage basin (see, e.g., our Fig. 10). Therefore, we strongly disagree with the reviewer's statement that using the SIA for modeling this region is not valid from the outset.

The shallow-ice approximation has been developed 30 years ago and has been extensively used …

Not everything that is old is bad! For example, Newtonian mechanics has been around for 300 years, and, even though we learned about its limitations over time, it is still a very useful tool and being used extensively. The negative ring this half-sentence conveys is therefore not appropriate.

… but is known to be valid only on slow moving areas where the motion is dominated by vertical shear. Fast flowing glaciers are dominated by basal sliding and lateral shear cannot be neglected as it provides significant resistance to the flow. It was therefore expected that the shallow-ice approximation would not perform well compared to full-Stokes on this glacier.

No, this is not correct. We would agree if we only modelled the fast-flowing Shirase Glacier itself, but, as stated above, this is not the case. Our modelling study concerns a significant part of the Antarctic ice sheet, in which the majority of the ice flows rather slowly and exhibits a slip ratio of < 0.5 (that is, the amount of basal sliding is limited; confirmed by our Figs. 8 and

9). Under such conditions, we think that it is an interesting and valid test to check how the SIA performs compared to full Stokes (FS).

The great strength of SIA is its simplicity and enormous computational efficiency. Even in connection with the shelfy stream approximation as hybrid models, SIA is often the only viable alternative in terms of computing resources for applications covering large spatial and temporal scales, such as paleoclimatic runs of an entire ice sheet. Showing by a case study, like ours for the Shirase drainage basin, where exactly the deviations of SIA results from FS exceed an acceptable margin is something that adds valuable information to potentially existing simulation challenges.

The conclusion of this paper suggesting that "careful consideration must be given to the representation of ice flow physics when attempting to model the dynamics and evolution of ice sheet areas containing ice streams and outlet glaciers" is not novel.

We are fully aware that this particular conclusion is not new, but rather a confirmation of previous findings. That's why we started the sentence with "This confirms that…". In order to make it even clearer, we have changed it to "This confirms findings of earlier studies that…" (page 1, line 10), and we have deleted the last paragraph of the conclusion.

Comparing different ice flow approximations is not new, and has been studied for at least a decade, on a number of idealized geometries (Hindmars,, 2004; Gudmundsson 2008) and real glaciers (Morlighem et al., 2010; Seddik et al., 2012; Furst et al., 2013), so the domain of validity of the different stress balance approximations is well known and there is nothing new added in this paper.

We did not wish to imply that our paper is the first ever in which different force balances have been compared, and we apologize for not having mentioned more of these earlier studies in the introduction. However, the statement that "there is nothing new added in this paper" is definitively not true. As the reviewer points out, Hindmarsh (2004) and Gudmundsson (2008) dealt only with idealized geometries. Seddik et al. (2012) compared FS and SIA for SeaRISE-Greenland scenarios, but used two different models, namely Elmer/Ice for FS and SICOPOLIS for SIA, which limits the comparability (because differences can also arise from different numerics etc.). Fürst et al. (2013), also a study on the entire Greenland ice sheet, used only one model, but the five approximations to the force balance are between (and including) SIA and Blatter–Pattyn (aka first-order approximation), thus excluding FS.

The study by Morlighem et al. (2010) is probably the most similar one to ours because it dealt with a part of Antarctica, used different force balances within one model and a control method to infer basal drag. However, there are still major differences. It dealt with a much smaller area, namely the Pine Island Glacier and its immediate vicinity. Therefore, the characteristics of their domain is very different from ours in that it contains a much larger fraction of fast-flowing ice. For such a domain, the SIA would be clearly inappropriate. Consequently the authors didn't use it and compared FS, Blatter–Pattyn and the shelfy stream approximation. Further, the study only investigated present-day stress and velocity fields, while we also discuss time-dependent future climate scenarios.

In short, our study is (to our best knowledge) the first in which FS and SIA are compared within one model for an application to a large area that has the characteristics of an entire ice sheet (slow flow in the interior [= largest part of the area], fast flow near the grounding line). We think that this is sufficiently novel to make our paper a valuable contribution, and we have revised the introduction significantly to make this point clear.

In addition, we would like to point out that, beyond the FS-to-SIA comparison, modelling the Shirase Drainage Basin in 3D with FS is novel in itself, and it is relevant because the area is a focus of Japanese research activities in Antarctica. Some findings of the FS simulations, all reported in the paper, are: (1) the observed surface velocity distribution can be reproduced well (Sect. 4.1), (2) the basal friction generally decreases towards the grounding line (in line with expectations; Sect. 4.1), (3) the simulated and observed present-day net mass balances agree within the observational uncertainty (Sect. 4.2), (4) the sensitivity of the Shirase Drainage Basin to SeaRISE-type basal sliding and surface climate experiments is similar to the sensitivity of the entire Antarctic ice sheet (Sect. 5). These findings are interesting in itself and will also attract the attention of colleagues who are just interested in the dynamics of the region, even if they do not care so much about the FS-vs.-SIA story.

Finally, the simulations performed in this manuscript rely on the Elmer/Ice software, that was recently used to develop a dynamical coupling between full-Stokes and the shallow-ice approximation (Ahlkrona et al., 2016). Applying this new coupling method to the Shirase Glacier and comparing its performance and accuracy to a more traditional full-Stokes model would have been of greater interest for this study.

Frankly, we think that this suggestion goes a bit too far because it would be something more or less completely different. Anyway, we hope that our above arguments are sufficient to explain why we did what we did, and why we think that our study is interesting for the community.

Reply to RC2

The manuscript provides a valuable examination of the ice sheet volume change for an East-Antarctic basin under selected scenarios from the SeaRISE effort, using two formulation for ice dynamics: Full-Stokes (FS) versus Shallow Ice (SIA).

General comments:

The strength of the study is that it attempts to maximize the similarities between the numerical simulations: the mesh is the same, same distribution of basal friction coefficient, same model (Elmer/Ice) etc in order to allow a clean comparison between the FS and SIA solutions. The major finding is a confirmation that the choice of ice dynamic will impact the ice volume evolution. Although this is not ground breaking, the value in the work is that it is a step towards understanding the sources of uncertainty in ice sheet evolution and hence sea level projections.

The manuscript is very well written and has a clear structure. The discussion and conclusion addressed many of the questions that came to my mind when I was reading the results. The tables and figures used are necessary and well prepared (apart from what is noted in the minor comments).

We wish to thank the reviewer for his/her efforts, and for the positive assessment of our work.

My major criticism is that the study would have been more interesting/complete/valuable if additional solver that seems to be available within the Elmer/Ice toolkit had been used too. In particular since it is recognized in the community that SIA is not ideal for capturing Antarctic ice sheet dynamics. Nonetheless the authors do acknowledge this limitation and the point is raised in the discussion as further work.

Along this line, we would prefer to leave it with the current FS-vs.-SIA comparison. Of course, any scientific study has room for adding more experiments/analysis etc. However, we feel that what we have now should suffice to be of interest for the community.

Minor comments:

P8, L10: Could you add an explanation of the need to set a minimum thickness of 10 m?

A minimum thickness is required in order to avoid having 2D (rather than 3D) elements. 2D elements would be treated as degenerated elements during the finite element assembly, so that the assembly would fail. The reason why we chose 10 m is to avoid a too low aspect ratio (thickness-to-width ratio) of the finite elements, which can cause the numerical solution to become unstable. We have added this explanation at the end of Section 2.4.

Figure 1: You have space in the figure to write “Fuji” in full.

Done (now Fig. 1a).

Figure 4 caption: can you improve the caption for the readers that like to look at figures & caption without having to dig in the text for understanding? Ie: the main text explains what the axis are but the caption by itself is not very meaningful.

We have extended the caption to make it more self-explanatory (now Fig. 2).

Figure 6: either in the figure or in the caption: can you define what the slip ratio is? It is defined in the introduction, but a reader may have forgotten about this.

We have added the definition in the caption (now Fig. 4).

Further changes

In response to an earlier comment from the scientific editor, we have reduced the number of figures by combining the previous Figs. 1–3 into the new Fig. 1.

We have changed the symbol used for the Clausius–Clapeyron constant in Table 1 from β to $\Delta T_\mathrm{m}/\Delta p$ in order to avoid the double use of β (denotes also the basal friction coefficient).